

I am against the proposal in this general article that Article 75.3.6 should be waived in relation to ciliates, other protists and small Metazoa. Successive editions of the Code have regarded the designation of neotypes as unusual acts, admissible only in exceptional circumstances. Therefore, rules governing the designation of a neotype are numerous and stringent, designed to ensure that a neotype will come as close as possible to the original concept of a nominal species.

Foissner proposes to relax the rules governing the designation of neotypes for Ciliophora and other groups of protists, and possibly even small Metazoa. As far as I am concerned, this proposal is unacceptable because it goes against both the letter and the spirit of the Code. It threatens the universality of the Code by trying to create a special interest group. Very few, if any, taxonomists have the luxury of working with a group that is free of nomenclatural problems. If we were to accept the logic of Foissner's proposal, there is a danger that each generation would be entitled to throw out the types prepared with 'outdated' techniques and allow 'authoritative' redescriptions to be made and supported by new neotypes. Advances in techniques and methods are valuable aids for taxonomy and accompanying nomenclature, they cannot be allowed to steer or control taxonomy and nomenclature. Relaxing the requirements of the neotypification process for ciliates while maintaining these same requirements for all other organisms would destroy the universality of the Code. Furthermore, it carries the very real possibility of students of other groups also making applications to waiver or to relax other requirements of the Code to facilitate their endeavours. That would destroy the universality and the authority of the Code.

Comment on the proposed precedence of *Ovula gisortia* Passy, 1859 over *Cypraea coombii* J. de C. Sowerby in Dixon, 1850 (Mollusca, Gastropoda)
(Case 3220; see BZN 59: 173–175; 60: 218–220)

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I disagree with the point of view of Todd (BZN 60: 218–220) on the following points:

1. The type material of *Cypraea coombii* only contains the complete specimen figured by Sowerby in Dixon (1850) and several remains. The specimen no. 5 mentioned by Schilder corresponds to *Gisortia tuberculosa* (Duclos, 1825) from the Ypresian of the Paris basin. I also stress that Edwards recorded a specimen which was originally deposited in the Museum of Bowerbank, but which was destroyed. This specimen possessed a large callosity on the dorsal face that differed from that of the type of *G. coombii*. This feature makes *G. coombii* closer to *G. gisortiana* than *G. tuberculosa*, as supposed by the authors who had erroneously regarded *G. coombii* as a variety of *G. tuberculosa*.

2. Todd disagreed with the use of the application of Article 81.2.3 of the Code for reasons which are unclear. When he referred to the geographic and stratigraphic

ranges of *G. coombii* he only confirmed that the stratigraphic ranges of *G. coombii* and *G. gisortiana* are identical (Early Lutetian). In addition the range and rarity of the specimens cannot be used to discuss the validity of the names. *G. coombii* is of course a rare species, but *G. gisortiana* is also rare in France.

3. Todd regarded the work by Schilder (1930) on *Gisortia* as the most complete treatment. Luc Dolin (world specialist of the cypraeids) and I do not consider that this work is the best work for the systematic treatment of *Gisortia*. Considering *Gisortia* and *Megalocypraea* (= *Gisortia*), Schilder discussed 35 taxa of which only five have a preserved shell; thirty taxa are preserved as internal moulds lacking useful morphological characters. This treatment is totally absurd in introducing many species, for which distinctive characters at generic level (e.g. terminal folds, fossula) as well as specific level are missing and only exist when the shells are well preserved. Thus, it appears that in his systematic treatment, Schilder discussed one of the rare specimens of *G. coombii* which has the shell preserved. Moreover, we also point out that the synonymy list presented by Schilder is very short.

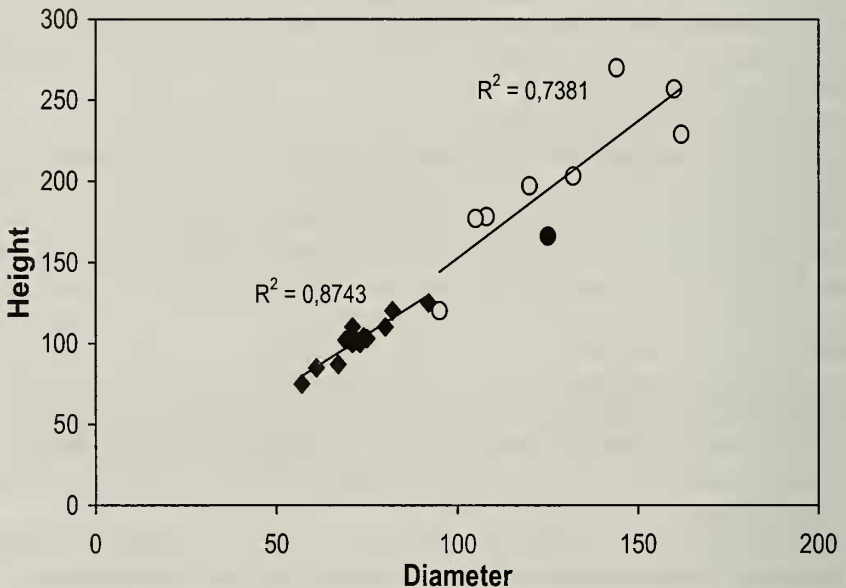
4. Among six reasons given by Schilder for the systematic treatment of the family, Todd remarks that two (Schilder's numbers 4 and 6) are relevant to the current application. Reason no. 4 (many species are known from only one or a few specimens) cannot be used as an argument against the synonymy of *G. coombii* and *G. gisortiana*. Moreover, this argument effectively favours the splitting of species. If Reason no. 6 (most writers have had no opportunity to examine original specimens) is pertinent for previous authors, it is not so in our case. We have examined the type specimens of all species attributed to *Gisortia*: *tuberculosa*, *coombii*, *gisortiana*, *pterochora* and *chevallieri*. The other type specimens are useless internal moulds. In addition, we also have the opportunity to check the type specimens of the subgenus *Vicetia* for further comparison. Consequently, I consider that we are able to give an accurate statement of the synonymy of *G. coombii* and *G. gisortiana*.

5. The short biometric analysis based on preserved shells (see figure below) justifies the taxonomic distinction between *G. gisortiana* and *G. tuberculosa*, and also demonstrates that *G. coombii* is closest to *G. gisortiana*. Also, for qualitative characters, the species strongly varies and *G. coombii* could be easily included in the range of variation of *G. gisortiana*. The callosities mentioned by Todd cannot be regarded as discriminant characters. The French specimens bear either very thick or very thin callosities and some specimens lack callosities. We have not only compared the available specimens using the size, but we have also used the variation of qualitative characters.

6. In our original manuscript we stated that *Gisortia gigantea* should be regarded as a nomen dubium. However, this point was not included in the published application. For further information, I stress here that *Conus gigantea* Keferstein, 1828 and *Conus gigantea* Quenstedt, 1836 are nomina nuda (see Schilder & Schilder, 1971). *Strombus giganteus* Goldfuss, 1841 (based on an internal mould) should be regarded as a nomen dubium as well as *S. giganteus* (see Shalfhaütl, 1863). Unfortunately, Dolin & Dolin (1983) regarded the taxon *G. gisortiana* as a synonym of *G. gigantea*, but we now regard this nomenclatural act as an error. Todd cited this work in taking the example of Vredenburg (1927). Dolin and I recognise that the

taxon *G. gisortiana* was often considered as a synonym of *G. gigantea* or that *G. gigantea* was regarded as a distinct species. In addition, Schilder (1927) erected three species of *Gisortia* including four subspecies of *G. gigantea* based on internal moulds. What is the scientific credibility of these taxa?

7. In conclusion, I hope that our proposition is not as premature and unsubstantiated as suggested by Todd. Regarding the occurrence of the taxa, it is also unrealistic to believe that a study based on numerous specimens can be made, only a few specimens being discovered in 180 years. Thus we do not understand why Todd disagreed with our proposal to apply Article 23.2 in order to conserve the taxon *G. coombii*.



Measurements of *Gisortia tuberculosa* (black diamond); *G. gisortiana* (white circles) and *G. coombii* (black circle).

Comment on the proposed conservation of usage of the specific names *Libellula aenea* Linnaeus, 1758 (currently *Cordulia aenea*) and *L. flavomaculata* Vander Linden, 1825 (currently *Somatochlora flavomaculata*; Insecta, Odonata) by the replacement of the lectotype of *L. aenea* with a newly designated lectotype (Case 3253; see BZN 60: 272–274)

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I support this application fully. It is a completely convincing and acceptable approach to providing nomenclatural stability.